

Baker, F.R.S., of Kew Gardens. Mr. Lucas is by no means backward in acknowledging by whom he has been aided in the completion of the work, and amongst others there are numerous and valuable contributions by Mr. J. R. Dakyns, M.A., Cantab. (of H. M. Geological Survey), both in the foot-notes and in the text.

Notwithstanding this, however, the book is an original work, everywhere bearing abundant evidence that the materials have not been compiled, but in great part collected upon the spot, and carefully worked out by the author himself. And as there are many secluded valleys in Cumberland, Westmoreland, and Yorkshire, in which the customs, manners, and folk-speech differ very little from that of Nidderdale, we think the volume deserves a much wider circulation than in the district of that valley from which it takes its name. Six of the concluding chapters are devoted to the birds of Nidderdale. These chapters on natural history are the most pleasing in the book, and contain information respecting the distribution of many birds which is altogether new. After these there is a well-told story in the dialect ("Dicky and Micky Date") by Thomas Thorpe.

Probably the most valuable, and certainly the most laborious portion of the work, is the glossary of the dialects of Nidderdale, with which it concludes.

Local glossaries no doubt there are without number, of the northern dialects, but we have never before seen one which has traced with such clearness, both from its use and derivation, each word to its source. A residence of over forty years in some of those remote regions in which a corresponding dialect is spoken, enables us to testify that Mr. Lucas has been wonderfully accurate and exhaustive in laying hold of the vocables of the district; and the pains and skill with which he has traced them through the Norse and other cognate languages, must be seen before they can be properly understood. T. E.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

The Sun-spot Period

THE sight of my *bête noire*, that part of Wolf's sun-spot curve lying between the years 1766 and 1799, so clearly plotted in the communication by Prof. Stanley Jevons, on "The Solar-Commercial Cycle" (NATURE, vol. xxvi, pp. 226-28), impels me to offer some remarks having special reference to solar periodicity at that time.

In a paper read at the meeting of the British Association in York last year, I ascribed the sun-spots to planetary tides in the solar atmosphere. It is not pretended that what was advanced amounted to demonstration, but the assumption had this practical result—it led me to the conclusion that the sun-spot maxima and minima, recognised in what is known as the sun-spot period, are, on the whole, determined by the relative positions of the planets Venus, Earth, and Jupiter. The maxima are nearly always associated with configurations in which Venus and Earth in conjunction or opposition, have Jupiter in or near syzygy or quadrature; while the minima are even more certainly associated with configurations in which Venus and Earth in conjunction or opposition, have Jupiter in or near the octant.

There are, however, significant deviations from this general law, and the maximum to which Wolf assigns the date 1788-1 occurs at a time when the law would give a minimum. Now it may be admitted, that at times special conditions prevail, arising from changes within the sun itself, or from the advent of material agglomerations foreign to our system. I prefer, nevertheless, to assume for the present, that the explanation of such periodicity as has been established is within the resources of a planetary hypothesis. Accepting the sun-spot record as read for

us by Prof. Wolf, because we have nothing better, it is inferred that the apparent anomalies of the period in question are due to exceptional planetary configurations.

The following statement shows how lamentably observation and theory are at variance, in regard to the sun-spot numbers, near the dates 1778 and 1789:—

Years of Maximum Annual Sun-Spot Numbers

Observation				
1761	1769	1778	1789	1804
Hypothesis				
1761	1771	1783	1794	1804

The remarkable series of corn-prices, as given by Prof. Jevons, however shows maxima so fairly in accord with the hypothetical maxima that I am tempted to quote them:—

Years of Maximum Corn-price at Delhi

1763	1773	1783	1792	1803
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If this relation is anything more than a coincidence, an important question arises. Are we to consider the sun-spot record defective, and reject the maxima of 1778 and 1789, because they cannot be traced in the corn-prices? Not necessarily, it seems to me. The sun-spot record may not be reliable, and with its revision difficulties may vanish, but there is something very substantial about the maximum of 1789, and it must be remembered that it is one thing to measure a sun-spot, and quite another thing to use a sun-spot as a measure. The sun-spot tells of solar disturbance, but the attendant changes in solar radiant forces will be changes in quality as well as in quantity, and it may be taken for granted that there are solar periods that are not to be found in the sun-spot numbers directly. One outcome of the researches of Dr. Köppen has been the recognition of what is called the period of the "Umkehrung," or inversion, so named because the more usual relations of sun-spots and air temperatures are supposed to be reversed during this particular period, which lies between the years 1770 and 1816, or thereabouts. Double-edged weapons are, however, dangerous, and must be used with caution.

Sun-spot measurement itself is a somewhat arbitrary process. The "relative number" for a given day is ten times the number of groups, plus the number of individual spots; while the method initiated by the Kew observers, and now adopted at Greenwich, gives "spotted area," that is, the proportion of the sun's surface covered by such spots as may be visible on that day. It would be interesting to compare the positions of the great spots seen in April last, as given on the annual sun-spot rolls at Zürich and Greenwich respectively. Moreover, certain well-marked distinctions in the character of the disturbance have no place, or next to none, in sun-spot measure—the faculae are ignored, while umbra and penumbra are lumped together.

It should be remarked that observation and hypothesis agree in the total number of periods, so that, the length of the mean sun-spot period remains unaltered, unless it is decided that certain observed maxima may be taken in addition to the hypothetical maxima, and not as replacing them. The planetary hypothesis requires that the sun-spot series shall be considered as a compound series, representing a number of more or less important series of planetary periods, and it is to be expected that at times there will be a difficulty in tracing any dominant series of periods, whether primary or derivative.

It seems to me that too much importance is apt to be attached to the mean sun-spot period, seeing that its occurrence is exceptional, and the departure from it very considerable.

That these observations should be inconclusive is a matter of course, but my purpose will be served, if they tend to produce the impression, that there may be no real solution of continuity in the relation between the sun-spot numbers and the particular series of planetary periods that I believe to give "the sun-spot period" a rational basis.

F. B. EDMONDS

72, Portsdown Road, London, W., July 14

Messrs. McAlpine's Atlases

WILL you allow me space in your columns to make a few remarks upon the "Biological Atlas" of Messrs. D. and A. McAlpine, and the "Zoological Atlases" of the first of these gentlemen?

Mr. D. McAlpine was, some three or four years ago, a student

in the biological laboratory at South Kensington, and, after a diligent attendance at Prof. Huxley's eighty odd lectures, and at the five months' practical work, he succeeded in passing the examination in the second class. The two following years Mr. McAlpine, with laudable perseverance, again presented himself for examination, each time appearing a place or two lower in the second class.

While working at South Kensington Mr. McAlpine made several copies of the diagrams of type dissections in the laboratory, which diagrams are for the most part enlargements of my original drawings made by my friend and former colleague, Mr. G. B. Howes. I naturally imagined that Mr. McAlpine, like other students who had taken the same trouble, intended to use these copies either for his private work or for his classes in Edinburgh, and I was, therefore, greatly surprised at the appearance of the Biological Atlas, to find in it a number of marvelously inaccurate copies of these same diagrams, published not only without permission, but without the slightest reference to their source even in the preface.

In the Zoological Atlas (Vertebrata) the same thing occurs, and my diagrams, although strangely altered, are quite recognisable; in the figure of the skate's nervous system, for instance, I notice, copied with unusual accuracy, a mistake as to the origin of the orbito-nasal nerve, which occurred in my original drawing, but which has subsequently been corrected.

In the cases where Mr. McAlpine, having no diagrams to copy, has had to depend upon his own dissections and the statements in text-books, the results are sometimes remarkable. As an instance, I may take the ingenious diagram of the skate's vascular system, in which *paired* caudal veins are shown accompanying the caudal artery, and passing directly into the corresponding cardinal veins, the renal portal systems being completely suppressed.

According to the advertisements, the *Athenæum* recommends the "Biological Atlas" to all students of the subject; I regret that I cannot agree with your contemporary; in my opinion no books could possibly be more mischievous to a beginner than these, since they hold up for his example and imitation a work of the most inaccurate and slovenly description; as indeed, if possessed of ordinary powers of observation, he cannot fail to find out for himself before he has been a month at the subject.

T. JEFFERY PARKER

Otago University Museum, Dunedin, N.Z., March 24

Palæolithic Implements—New Localities in the Thames Valley, near London

IN NATURE, for July 15, 1880, p. 253, Mr. P. H. Pepys drew attention to a section then being made through beds of river gravel and brick earth near the West Drayton Station of the Great Western Railway. I had an opportunity of going to West Drayton on July 27, 1880, so I walked through the cutting towards Langley. My quest was for relics of primæval man, and I was rewarded by finding not only several flint flakes, but the butt end of a massive implement broken in Palæolithic times. This was just north of Langley Station, in Buckinghamshire, and the first Palæolithic relics, as far as I know, detected in that county. The workmen in the cutting for the new canal were such a rough lot that I found it impossible to fraternise with them, so my subsequent visits were all made on Sundays. During these walks I lighted on ten implements and a large number of flakes at Langley and Iwer, all in the valley of the Coln, and a river until now (as far as I know) not described as implementiferous. In gravel brought from the pit close to Taplow Station I found a single implement, a large trimmed flake, and numerous simple flakes; this position is also in the county of Buckingham. At West Drayton, in Middlesex, in the valley of the Coln, I lighted on five implements and numerous flakes. East of West Drayton, in a pit near Botwell, in the valley of the Yedding Brook, hitherto undescribed as implement-bearing, I found a single implement; this was in the pit near Bull's Bridge. In the same valley at Hillington, and other places I have found several other implements. In all the excavations from Slough to Acton I have found both implements and flakes. In the new railway cutting from Gunnersbury to Hounslow I have found four implements, one close to Hounslow, a massive butt, and many flakes. This cutting has been a very interesting one, from the abundance of the fossil shells of fresh-water molluscs found in the sands, especially near the bridge under the Hanwell Road. One shell very abundant, and, as far as my observation goes, absent from the sands of

North-east London, is *Achatina acicula*, Müll., kindly named for me by Dr. J. Gwyn Jeffreys. I believe this is the first record of fresh-water shells from the Palæolithic sands of the Ealing district. Since my paper on the Valley of the Brent was read before the Anthropological Institute, in June, 1879, I have found many more implements in the positions there mentioned. At North-east London, and especially in the Valley of the Lea, I have been able to greatly extend the range of Palæolithic man. In addition to the localities mentioned in my paper read before the Anthropological Institute, in June, 1878, and published in February, 1879, I am now able to mention London Fields, Homerton, in the south, a position south of Dalston Junction, and nearer the Thames than the places first given by me, Hackney, near the railway station, Abney Park Cemetery, South Hornsey, Highbury, Stamford Hill, Upper Edmonton, Lower Edmonton, Bush Hill Park, Forty Hill, Enfield, and Cheshunt; the pit at the last place, which formerly produced flakes only, has since furnished three implements—one an example of the first class. On the east side of the Lea I have found implements in the gravels of Stratford, Leyton, Leytonstone, Wanstead, Walthamstow, and Higham Hill—a magnificent example from the last place. Further east, and in the Valley of the Roding (first pointed out by me as a river bearing implements in its gravels)—at Barking—I have found two implements, and elsewhere in the neighbourhood, as at Ilford and Upton, numerous flakes. Still further east, at Gray's Thurrock, West Tilbury, and Southend, I have evidence of the presence of primæval man; at the latter place, a rude make-shift implement, and a scraping-tool with twin bulbs of percussion. These were found by my two sons. I have not mentioned all the positions I know in this letter, or re-mentioned those given in my two papers, but rather the positions I can afford to dispense with. It shows, however, especially when considered with the discoveries at Reading and Oxford, what a vast cohort of men once lived all along the Thames and its northern tributaries in Palæolithic times.

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"Halo": Pink Rainbow

THE appearance noted in NATURE this week (p. 268) by Prof. O'Reilly must surely have been a case of the *rayons du crépuscule* that are frequently visible near sundown in the eastern sky. East-south-east cannot at this season be very far from opposite the setting sun. Prof. O'Reilly does not mention, though probably it was the case, that the point of convergence of the "beams" which he saw was diametrically opposite the sun's position. That these beams appeared dark is probably merely caused by the real "rayons" being wide, with narrow, darker interspaces between. I have several times (see *Phil. Mag.*, 1877) called attention to the existence of similar rays crossing the rainbow radially; indeed, it is seldom that a rainbow occurs when the sun is low in the sky, without one or more such rays being visible within the arc. Two such rays, for example, were visible in a bow seen here at sunset two evenings ago. This bow was interesting in another way also; for, like the "pink" rainbows about which there was some correspondence in NATURE last year, the only colours visible (in the primary arc) were red and yellow, the red being of a pinkish rather than a crimson hue.

SILVANUS P. THOMPSON

Pollokshields, Glasgow, July 20

Smoke Abatement

COUNT RUMFORD founded the Royal Institution of Great Britain nearly a hundred years ago, chiefly, I believe, to introduce improved grates, fireplaces, stoves, &c., as he then foresaw the necessity of economising coal and obtaining more complete combustion.

In about the year 1860 Faraday himself showed me Count Rumford's models, &c., and some of Rumford's working stoves in the model-room in the Institution, a subject in which I was then much interested, as I was enlarging my own house.

About ten years ago, when the laboratory of the Royal Institution was enlarged, the models, stoves, &c., devised by Count Rumford were removed. It would be important to know what has become of them. Would you kindly allow me to ask this question?

A MEMBER OF THE ROYAL INSTITUTION

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